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the hard and fast line of 85° north latitude, where the ice-cap is supposed to be fixed by centrifugal and other forces, and insists on my saying I will have a smooth ice-cap to travel over to the pole; whereas, in both cases, *about* 85° north and a *comparatively* smooth ice-cap are intended.

"And in returning, he intends to use the southern drift of the ice," etc. I speak of, and lean upon, *probabilities* in my theory altogether; and the millions of square miles of ice that drift out of the Arctic Ocean do not come from near the pole. Capts. Nares and Markham, nor any one else, has ever seen a 'paleocrystic sea of ice.' All the ice ever witnessed by the eye of man has been 'broken floe' or drifting 'pack' of but one or two years' growth, or at most of but a few years' growth, that breaks up and drifts away from the outer edge of the ice-cap under discussion.

While drifting in the Jeannette, we observed a large 'old-time' floe piece (about sixteen miles area) that might have been of any age. It certainly was very much older than any surrounding ice; and it had the appearance of what might be termed a *piece* of the paleocrystic ice-cap near the pole, that had been broken off farther up into the upper 'fringes' of the cap, and had drifted away.

This floe piece has no reference to bergs that are made upon the land, and take ages to form, but to the ice of the salt sea. Hard as dolomite, and of the dead color of marble, it was entirely different from the pack and floe ice of 70° or 80° north latitude. This is one more reason why I believe, that, as we proceed toward the pole north of 85°, we should find the disputed ice-cap formed of this older ice.

Dr. Boas says, that, "if it can be proved that an ice-cap cannot exist, his [my] plan must needs fall to pieces. . . . No doubt the centrifugal pull at a certain parallel will be equal on every meridian," etc.

But the very mile of ice that Dr. Boas depends upon to pull the ice-cap from its place, and hurl it down toward the equator, is just the mile or miles of ice that break away from the ice-cap because of the weakness of the ice at the fringes; and, as it breaks away, the cap regains its equilibrium. Whether the strength of the ice at the pole will withstand the centrifugal force, tending to carry it toward the equator, will depend upon the velocity of that force, and the amount of hold and the number of holds the islands have upon the cap, as well as the contending currents and other forces that are continually at war with each other. And I do not concede that my argument referring to an equal pull (comparative and then equalized again) by the centrifugal force cannot be maintained.

Even though all my theories fall to the ground, I am doing just what Dr. Boas wants his readers to believe I am not doing; that is, I am not depending upon my theories alone for my scheme of advance toward the pole, but I am depending upon the lessons taught by 'former experiences,' and 'not on vague theories.'

I propose to go by way of Franz Josef Land, for the reasons that it is the northernmost land known on the face of the earth; it is readily accessible every year; retreat from this land is sure and safe. By this route alone are we enabled to hold to the land to the farthest point north, and I would not leave the land at all if it extends all the way to the pole.

But if it does not extend all the way to the pole, then I must take to the ice, over which I believe I will have easy travelling, compared to that south of 80°, where the motion is greater than toward the pole, and where most of the observations of the ice conditions have been made. All, all else is theory.

I have no desire to go to the Arctic to perish. It is my knowledge, founded on personal experience and that of others that I have studied intently, that makes me believe that I can go to the pole *via* Franz Josef Land with a greater degree of safety than by any other route, and *that it is the route of the future.*

I thank Dr. Boas for his courtesy and words of praise. I do not class him with the narrow-minded obstructionists of whom he speaks; but I cannot agree with his hair-splitting arguments, or his hard and fast lines of demarcation, in which he insists that my theories are based on erroneous data, or in opposition to the standard authorities of the day.

GEORGE W. MELVILLE,
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Columnar structure in sub-aqueous clay.

During the summer of 1883, in the vicinity of Menomonee, Dunn county, Wis., I was fortunate enough to see, while it was still fresh, a deep railway cut through the sub-aqueous clays which overspread that region, reaching up to considerable altitudes above the Red Cedar River. The cut was something less than a hundred feet above the stream, and between twenty-five feet and thirty feet deep. Beneath the shallow soil was a stratum of distinctly laminated brown-yellow clay-loam about ten feet in thickness. Beneath this was a stratum of clay of a peculiar greenish hue, also distinctly laminated, and through which occasional sandy partings were traceable. This stratum was about five feet in thickness, and was followed, in descending order, by stratified sand, which extended to the bottom of the cut.

In the second clay stratum, reckoning from the top, columnar structure was beautifully developed. Not only was this structure conspicuous as seen in the nearly vertical face of the cut, but several of the hexagonal columns had individually separated from the others, and, after falling a number of feet from their natural position, still preserved their integrity as they lay at the bottom of the cut. The columns varied in diameter from ten to fifteen or sixteen inches. They were uniformly, but not regularly, six-sided, and could be divided easily across their longer axes, parallel to the bedding planes, so that each column was separable into regular sections. Whenever this parting was made (and the experiment was repeated several times), the opposing surfaces, after separation, were never plane, but always showed a greater or less curvature, convexity fitting concavity. If my memory serves me rightly, the convexity was at the lower end of each section, though, unfortunately, I find nothing in my notes on this point.

Another interesting feature was observable on the cross-section surfaces; viz., a distinctly concentric structure. This, in some cases, was very conspicuous; in others, to be discovered only on close inspection. In more than one case, the concentric lines, which were real structure-lines, and not merely lines of coloration, were almost continuous around the column, but they were more commonly somewhat interrupted. The concentric lines were generally very numerous, and therefore closely approximate, and more commonly best developed just about the centre, or else near the exterior of the column.

This structure would seem to be an additional confirmation of the hypothesis which ascribes columnar structure to concretionary action.

Beloit, Wis., March 25. R. D. SALISBURY.